DOCUMENT RESUME

ED 114 877 CS 501 163

AUTHOR Berger, Charles R.
TITLE The Covering Law Model in Communication Inquiry.
PUB DATE 75
NOTE 31p.; Paper presented at the Annual Meeting of the Speech Communication Association (61st, Houston, December 27-30, 1975)

EDRS PRICE MF-$0.76 HC-$1.95 Plus Postage
DESCRIPTORS Behavior Theories; *Communication (Thought Transfer); Educational Research; Educational Theories; *Models; *Speech; *Theories
IDENTIFIERS Communication Theories; *Covering Law Model

ABSTRACT

The first section of this paper defines covering law explanation as a theory which maintains that explanation may be achieved, and may be achieved, by subsuming what is to be explained under a general law. The model is examined in light of the deductive-nomological explanation, the deductive-statistical explanation, and the inductive-statistical explanation. The second section of the paper examines the feasibility of covering law models and examines some criticism of the covering law approach, including laws versus rules, cultural variation, and temporal variation. The third section of the paper examines potential barriers to theory construction and discusses irrelevant variety, irrelevant change, realism versus nominalism, insensitivity to embeddedness, and overemphasis on statistical techniques. It is concluded that a defense of the covering law approach does not deny the usefulness of the rule governed approach or the systems approach, and that the covering law approach probably does provide the most complete explanation of a phenomenon when the explanans are true. (TS)

****
* Documents acquired by ERIC include many informal unpublished *
* materials not available from other sources. ERIC makes every effort *
* to obtain the best copy available. Nevertheless, items of marginal *
* reproducibility are often encountered and this affects the quality *
* of the microfiche and hardcopy reproductions ERIC makes available *
* via the ERIC Document Reproduction Service (EDRS). EDRS is not *
* responsible for the quality of the original document. Reproductions *
* supplied by EDRS are the best that can be made from the original. *
****

U S DEPARTMENT OF HEALTH
EDUCATION & WELFARE
NATIONAL INSTITUTE OF
EDUCATION
THIS DOCUMENT HAS BEEN REPRODUCED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT. POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL NATIONAL INSTITUTE OF EDUCATION POSITION OR POLICY

ED114877

THE COVERING LAW MODEL IN COMMUNICATION INQUIRY*

By

Charles R. Berger

Department of Communication Studies
Northwestern University
1822 Sheridan Road
Evanston, Illinois 60201

*The author would like to express his deep appreciation to Paul H. Arntson, Glen W. Clatterbuck and Gerald R. Miller for their helpful criticisms of an earlier draft of the present paper.

CS 501 163

THE COVERING LAW MODEL IN COMMUNICATION INQUIRY

Charles R. Berger

Northwestern University

During the 1950's and 1960's, persons interested in behavioral scientific approaches to the study of human communication concentrated most of their efforts on the task of acquiring and using various methodological and statistical techniques. In part, this was a healthy trend at the time; however, it turned many good researchers in our field into well trained hypothesis testers for theory builders in such disciplines as social psychology, sociology, psycholinquistics, sociolinguistics and political science. The role models for communication researchers in terms of theory construction were found in these and related disciplines.

In the past few years, there has been an increase in the number of papers, journal articles, and books concerned with either issues related to the process of theory construction in general or the presentation of substantive theories or models within communication (Berger and Calabrese, 1975; Cappella, 1974; Cushman and Whiting, 1972; Hawes, 1975; Monge, 1973; Parks, 1974; and Pearce, 1973). It is probably fair to say that only through the development of a unique body of theory will communication achieve a disciplinary status equal to the statuses of the other behavioral sciences. Obviously, methodology and statistical techniques do not define a discipline. Biology and sociology employed path analysis long before it was introduced to communication (Duncan, 1966 and Wright, 1934, 1954). The employment of various techniques in communication research, no matter how esoteric the methodology, will not serve to define us as a unique discipline.

If the primary task of present day communication researchers is to develop and test communication theories, it is imperative that they know what theory

generating options are available to them and what these various options have to offer. The alternative discussed in the present paper is one which has served as the dominant model in both the physical and social sciences: the covering law model. In the first section of the paper, the basic structure of the covering law model will be sketched. We will then examine some criticisms of the covering law approach. Finally, we will look at some issues surrounding the realization of covering law theories in communication inquiry.

THE COVERING LAW MODEL

The basic aim of scientific theory is to provide explanations for observed phenomena. Some might argue that theories also aim to predict; however, explanation takes precedence over prediction. If a theory can provide a complete explanation for a phenomenon, it can predict the phenomenon. By contrast, it is possible to have accurate prediction without explanation. In the case of scientific explanation, the paramount question for which the theoretical structure must provide an answer is the question "why?". Why do certain groups of persons have higher rates of interpersonal conflict than others? Why did a particular kind of message induce a large amount of attitude change? Why do persons engage in more self-disclosure in certain circumstances? These are some questions communication researchers might wish to answer. The only way that they can provide an answer for them is to build a theory. For we might be able to predict and demonstrate with 100% success the rule that the person who answers the telephone always says "hello" before the caller says "hello", but after observing the phenomenon 1,000 times, we would still not be able to answer the question "Why is it so?". Uncovering a regularity of behavior either in a "natural setting" or in a laboratory does not explain it. This issue will be explored in greater depth later in the present paper.

The term "covering law model" was introduced by Dray (1957) in his discussion of historical explanation. Dray defined the notion of covering law explanation as follows:

> To put it in a summary way, what the theory maintains is that explanation is achieved, and only achieved, by subsuming what is to be explained under a general law. Such an account of basic structure of explanation is sometimes referred to as 'the regularity analysis'; but because it makes use of the notion of bringing case under a law, i.e. 'covering' it with a law I shall often speak of it hereafter as 'the covering law model.'" (p. 1).

Dray cites Braithwaite (1953) and Gardiner (1952) as sources for his definition of the covering law model of explanation. For a number of reasons which will not be dealt with in the present paper, Dray rejects the position that covering law explanations are possible in historical inquiry. He argues that such explanations are likely to be misleading when one attempts to employ them in historical research.

A more detailed explication of the covering law approach was presented by Hempel (1965). Hempel distinguished among the following three kinds of explanations: Deductive Nomological (D-N), Deductive Statistical (D-S), and Inductive Statistical (I-S). Of the three, the D-N explanation is considered to be the pure case of the covering law model. However, Hempel asserts that both D-S and I-S forms of explanation contain elements of the pure covering law model. He considers both D-S and I-S paradigms of explanation to be at least partially subsumed under the general concept of covering law model. In the remainder of this section we will focus our attention on the characteristics of D-N explanation and show how it differs from the other two types of explanations.

Deductive-Nomological Explanation

A D-N explanation is divided into the following two general parts: (1) that which is to be explained or the explanandum, and (2) that which does the explaining, the explanans. The explanans, in turn, is divided into two groups of statements. The first type of statement contains particular facts or initial conditions under which the explanandum-phenomenon will occur. The second kind of statement expresses a uniformity by means of a general law. Taken together, the initial conditions and the general laws give rise to the explanandum. In other words, the explanandum can be deduced from the combination of the initial conditions and the general laws. Most theories contain a number of initial conditions as well as general laws. It is also assumed that the explanandum-sentence itself is true. Finally, when we refer to the fact that the explanandum is deduced from the explanans, we do not mean to imply that only syllogistic reasoning is employed in such deductive operations. In fact, most theories in the physical sciences use deductive systems which are far more complex than the syllogistic form of deduction presented in elementary logic books.

When we speak of laws in the case of D-N explanation, we are referring to general laws of the form "All X is Y." Laws of this type are also known as universal or deterministic laws. In the pure D-N system, it is assumed that the laws are true and not restricted by time or space (Nagel, 1963). That is, the law is assumed to have held in the past, to hold in the present, and will hold in the future. When we assert that every body that is near the earth and that freely fall toward it fall with an acceleration of 32 feet per second per second, we are advancing a general law which applies to all bodies falling toward the earth which are near the earth. However, while we may make the claim that such a law is "universal," it is probably the case that no law in any of the sciences meets the strict logical criterion of universality. In commenting on the "universal" character of laws Kaplan (1964) asserts:

> We might say instead, however, that a scientific law does not have the form of a strict universal, that this is only an idealized reconstruction. A universal proposition, such as might occur in pure mathematics, is indeed falsified by even one contrary instance. But in empirical science laws are enunciated and used even though contrary instances are known to be possible, or indeed, when some contrary instances are even known to occur (p. 96).

Not only are all laws likely to have exceptions but since it is impossible to test a law in all possible circumstances and at all possible times, it is not possible to erase all doubt about the possible truth or falsity of a law. Hempel (1965) has suggested that when there is doubt about the truth of a law, the term "lawlike" ought to be used. Braithwaite (1953) has made the same observations concerning the possible falsity of laws and has adopted the term "hypothesis" to show the tentative nature of most laws. In practice, then, D-N theories consist of a set of initial conditions or antecedent conditions, a set of lawlike generalizations, and a statement to be explained.

When we assert that a lawlike statement is "general," we do not necessarily mean that it applies to all possible instances of a given phenomenon. Theories have both explicit and implicit boundary conditions (Dubin, 1969). These conditions specify the domain of events the theoretical system is designed to explain. We might construct a theory, containing several general, lawlike statements, which is only meant to explain a phenomenon as it occurs among males or Americans. For purposes of evaluating the theory, we must confine ourselves to the intended domain. Moreover, if the initial conditions of the theory are not met in a particular test of it, the data adduced in such a study are of little use in evaluating the theory. Frequently, boundary conditions are explicitly stated by the theory builder; however, boundary conditions also may be determined through tests of the theory.

A simple example of a D-N system from Braithwaite (1953) might help to clarify the above discussion. Galileo's law concerning free falling bodies and the lower level hypotheses subsumed under it represent a deductive system

with hypotheses at three levels of generality. The hypotheses are listed from most to least general as follows:

Hypothesis I: Every body near the earth freely falling towards the earth falls with an acceleration of 32 feet per second squared. (By procedures of integral calculus the next hypothesis follows.)

Hypothesis II: Every body starting from rest and freely falling towards earth falls $16t^2$ feet in t seconds, whatever number t may be. (Employing the applicative principle the next hypothesis follows.)

Hypothesis III: Every body starting from rest and freely falling for 1 second towards the earth falls a distance of 16 feet.

We could continue to derive specific hypotheses concerning the relationship between elapsed time and the number of feet fallen during that time. For example, we would predict that every body starting from rest and freely falling for a period of 2 seconds towards the earth will fall a distance of 64 feet. We could then derive a distance prediction for 3 seconds and so on. All of these hypotheses would be at the same level of specificity and would constitute the lowest level hypotheses of the theory. Moreover, these lower order hypotheses would be the ones we would actually test either through observation or some kind of experimental procedure.

There are several characteristics of the above D-N explanatory system that should be noted. First, in the highest level law there is an explicit statement of a boundary condition; namely, that the law is only true "near the earth." Second, the lowest level hypothesis necessarily follows from the higher level ones; that is, the event must occur, within the boundary conditions of the theory, because it follows logically from the premises. Third, if the lowest order hypothesis in the system is not confirmed by experiment or observation, the entire theoretical system as it stands is disproved. However, this is not always the case with a deductive system. Braithwaite (1953 points out that if a lower order hypothesis follows from two or more higher level hypotheses of the same

level, refuation of the lower level hypothesis will lead to refutation of the conjunction of the two or more higher level hypotheses. At least one of the higher level hypotheses will be shown false by refutation of the lower order hypothesis. Thus, in the case of multiple higher order laws of the same level, complete refutationis not possible. Braithwaite (1953) goes on to assert that in deductive systems with multiple higher order hypotheses at the same level, it is possible to maintain any one hypothesis in the face of contrary evidence; however, this can only be done by modifying other hypotheses in the system. A fourth point worth noting in our example concerns confirmation of the lowest order hypothesis. In the example, confirmation of Hypothesis III would not provide complete support for the higher level hypotheses. If we were to continue to derive lower order hypotheses for different time periods and each hypothesis were confirmed through observation and/or experiment, the theory would become more fully supported. However, at no time would we ever be able to assert with complete confidence that the theory is "true" or "proved." This is the case since there will always be unexamined instances of the hypothesis. Complete confirmation of theories is not possible.

What typically happens to the kind of deductive nomological system examined in our example is that it is eventually subsumed by a higher order theoretical system. Brodbeck (1968), as well as others, points out that the higher order hypotheses or axioms of Galileo's theory became the lower order derived hypotheses or theorems of Newton's theory. Newton's theory, in turn, was eventually subsumed by Einstein's theory. Successive subsumption of one theory by another indicates one great strength of the D-N model. It is cumulative and leads to a systematic search for progressively more general laws with which to explain a larger number of empirical events. There are few examples of such successive subsumption

of theories in the behavioral sciences in general and there are no examples that I am aware of in the discipline of communication in particular. Perhaps successive subsumption of theories of more general theories is the hallmark of a "mature" discipline.

Deductive-Statistical Explanation

In both the physical and social sciences there are lawlike generalizations which are not deterministic in character. Instead, these generalizations are of a statistical nature; that is, the probability that an event X is also of the type Y is r, or that:

$$P(X,Y)=r$$

A statistical law asserts that in the long run the proportion of instances of the type X that are also of the type Y is approximately r. Both the laws of genetics and the laws of radioactive decay are statistical in nature. We can predict with a certain probability that if males and females with certain eye colors have large numbers of children, a certain proportion of those children will have a certain eye color. However, we are not in a position to predict what the eye color of a particular child will be. As Brodbeck (1968) has pointed out, statistical laws only allow us to predict particular collections of events; while deterministic laws permit us to make predictions of particular events.

By contrast to a D-N system of explanation, the deductive-statistical (D-S) mode of explanation contains at least one statistical law as part of its explanans. As the name of the explanatory system suggests, the D-S explanation is deductive in nature; however, because it contains at least one statistical law, the explanandum statement must include some kind of probability statement. In a D-S explanation, statistical probabilities which appear in the explanandum are derived from probabilities specified in the explanans through the mathematical theory of statistical probability.

Inductive-Statistical Explanation

A final type of explanatory system explicated by Hempel (1965) is that of inductive statistical or I-S explanation. In this case an effort is made to explain a particular event by employing statistical laws in the explanans. The following example illustrates this situation. Let us assume that we can make the statement that perceived similarity of attitudes between persons has a high probability of producing a high level of attraction between the two persons. Let us further assume that Dick and Jane have met and that they have similar attitudes. From this information we might conclude that it is very likely that Dick and Jane will be highly attracted to each other. In this example, the explanandum statement that it is very likely that Dick and Jane will be highly attracted to each other does not follow with deductive certainty from the statistical law that perceived similarity leads to interpersonal attraction with a high probability and the statement that Dick and Jane are similar to each other.

Since it is not possible to deduce the explanandum from the explanans with deductive certainty in the above example, we must construe the explanans as inductive support for the explanandum. To the extent that inductive support for the explanandum is strong, we can link the explanans with the explanandum with progressively stronger statements of certainty. However, there are additional problems with I-S explanations. For example, in explaining a particular event, it is possible to develop explanans with true premises which yield contradictory conclusions. Let us say that I encounter a person with a hot temper (T) who also has red hair (R). Upon consulting a social psychologist, I am told that it is not surprising that I should observe that such a person has a violent temper since the probability that a person with a violent temper will be redheaded is .90. Schematically we might represent this argument as

follows; where T_p stands for a particular person with a hot temper.

$$p(T,R) = .90$$

$$\frac{R_p}{T_p} \quad (.90)$$

In this argument, our statistical law states that the probability of observing both hot temper and redheadedness is .90. We observed a person with red hair (R_p) and therefore conclude with 90% certainty that the person has a violent temper (T_p).

Let us assume that if a person receives loving care from a significant other (L), the probability that the person will not display a violent temper ($\overline{T}$) is .85. Further assume that our redheaded friend did in fact receive considerable loving care from a significant other just prior to our meeting him. The following argument could be developed employing this information:

$$P(\overline{T},L) = .85$$

$$\frac{L_p}{\overline{T}_p} \quad (.85)$$

This argument states that there is a .85 chance that loving care will produce non-hot temper behavior. Since our redheaded friend received loving care, there is a .85 probability that he will not display a violent temper.

The premises of both example arguments are true but they lead to contrary conclusions about displays of temper in the individual case. It is not relevant that in the second case we were unaware that the person in question had received loving care just prior to our encounter with him. The argument itself is true and produces a contrary conclusion. Hempel (1965) refers to the above situation as "the ambiguity of inductive-statistical explanation." (p. 394) The purpose of including an example of this ambiguity in the present discussion was to alert

the reader to at least one of the difficulties with I-S explanations. Hempel discusses a number of ways of dealing with the problem, as well as other problems associated with the I-S explanatory system. It should be noted that the kind of ambiguity discussed in connection with the I-S system is not possible with a deductive system. If the premises of a deductive system are true, then so is its conclusion. A contradictory conclusion which is false cannot be the logical consequence of premises that are equally true.

THE FEASIBILITY OF COVERING LAW MODELS

The covering law model of explanation has been objected to on various grounds. In the following discussion we will examine some of these objections and attempt to ascertain their veracity. Not all possible objections to the covering law approach will be dealt with in detail in this discussion. Only major objections to the model will be examined. The interested reader should consult Dray (1957) for a critique of the covering law model in historical explanation. Hempel (1965) also deals with criticisms of the approach from both historians and scientists.

Laws versus Rules

A number of scholars from such disciplines as anthropology, sociology and social psychology have argued that since all or most of human behavior is rule governed, the covering law model of explanation, with its emphasis on the development of lawlike generalizations, should be abandoned in favor of a search for the rules which guide human conduct (Garfinkel, 1967; Goffman, 1959; Harré and Secord, 1972: and Toulmin, 1969). If one accepts the notion that human communication is rule governed rather than law governed, then the covering law approach or any other approach which relies on the formation of lawlike generalizations is not appropriate for communication inquiry. Harré and Secord (1972) have argued that at least for the time being, behavioral scientists should

stop examining variables and their causal relationships with each other and begin to study in detail episodes and the rules which guide behavior in these episodes. Given this argument, it would seem that not only would the rules approach dictate the abandonment of the covering law model, but such a paradigm would also demand that less general "systems analyses" involving variables (Monge, 1973) also be discarded. Thus, contrary to Monge's (1973) suggestion, if one views communication behavior as rule governed and accepts Harré and Secord's view, the kind of Blalockian causal analysis (Blalock, 1969) which Monge proposes as an alternative to covering law theory is clearly inappropriate for studying rule governed behavior.

Fortunately, Harré and Secord recognize that ultimately the concept of rules does little in the way of explaining behavior. They state:

> Though the concept of rule can be used to explain how an actor comes to know what to do, it still leaves open the question as to why he chose that rule to guide his conduct, nor does it explain why he actually acts on the rule, rather than doing nothing. In explaining action by reference to rule it is necessary to add some further accounts of wants, needs, or expectations of others, the awareness of which would prompt a man to action in accordance with the rule. Thus instancing the rule answers the question 'How did he know what to do?' but not the question, 'Why did he do this thing then and there?'. (pp. 181-182)

If a person explains a sequence of behavior by recourse to rules; that is, "The reason Person A in Situation B behaved in ways X, Y, and Z was because of certain rules he followed," the person offering the explanation runs the risk of commiting the error of hypostatization.

At least two questions can be raised concerning the laws-rules distinction. First, we can ask whether the covering law model is necessary for explaining why persons choose certain rules to guide their behavior in certain situations. The major thrust of the present discussion as well as the view given by Harré and Secord suggest that some kind of covering law approach would be necessary in this case. At some point one must go beyond mere description of "what the

rules are" and become concerned with why some rules are selected over others. Moreover, the problem of what kinds of environmental cues or situational attributes are responsible for the engagement of certain classes of rules would also seem to be a covering law question.

A second question concerning covering laws and rules involves the genesis of rules; that is, what social forces produced the kinds of conventions and "appropriate" modes of behavior we now observe? This question would seem to be approachable via the covering law model. Perhaps cultures with certain characteristics have greeting rules which are systematically different from cultures with a different set of characteristics. Such a statement is an obvious case of a lawlike generalization involving "variables." In addition to the question of genesis of rules, we mi lso ask questions concerning the processes through which rules are transm ed from generation to generation. Here again, covering law style approaches from learning theory and social modeling (Bandura, 1969) might be employed to explain such phenomena.

The above discussion suggests that while the notion of rules may be of considerable usefulness at the descriptive level, it is still necessary to develop explanations for various manifestations of rule governed behavior. Resort to such notions as meta-rules, meta-meta-rules, meta-meta-meta-rules, etc. does not seem to be a very parsimonious approach to the problem. Moreover, even if such a progression were developed, it is doubtful that it would adequately answer the "why" questions raised earlier. What is needed, then, is a small number of lawlike generalizations and a set of initial conditions which will provide explanations for the kinds of regularities observed under the rubric of rule governed behavior.

It is interesting to note that some of the researchers who advocate the rule governed approach frequently disavow the usefulness of the experiment as a research methodology (Harre' and secord, 1972). Instead, it is argued that such techniques as Garfinkelling are most efficient for determining

whether or not a particular performance is rule governed. In short, Garfinkelling is the practice of behaving in a way calculated to "break the rules" so that the consequences of rule breaking can be observed. To the extent that the social fabric of the situation is violated, one can be certain that a rule has actually been broken. According to Campbell (1975) Garfinkelling is little different from a quasi-experiment without randomization or control groups. Garfinkelling is equivalent to an experimental manipulation involving only one value of the independent variable!

The above theoretical and methodological considerations point to the conclusion that perhaps the rule governed approach and the covering law approach are best viewed as complementary rather than competing. As Harre and Secord (1972) point out, it is almost certain that some facets of social behavior, e.g. biological bases, will be best explained by recourse to some kind of covering law or causal analysis. Other facets of social interaction will be dealt with best for the present time by employing a kind of rule governed approach. However, it is important to keep in mind that ultimately the why question will have to be addressed by those who take the rule governed approach.

Cultural Variation

It has been argued by some persons that if it is assumed that communication is culturally bound such that symbolic behavior in one situation is not predictive of symbolic behavior in similar situations, it is not possible to establish laws. Thus, given cultural variation, the covering law model of explanation is inappropriate (Monge, 1973). Earlier in the present paper, it was pointed out that theories do specify boundary conditions and/or initial conditions.

Universal generalizations or lawlike generalizations are made within the context of such boundaries. Monge (1973) points to several physical laws which he claims are invariant through time and space and are thus universal. However, when one examines these physical laws carefully, he finds that they do not hold in all cases. Kaplan (1964) provides the following illustration:

> When a student first learning the gas laws asks why a gas doesn't completely disappear at a sufficiently low termperature, thus violating the law of conservation of mass, it is misleading to inform him only that 'we can't get to absolute zero'. It is more appropriate to make clear to him that the gas laws no longer apply, to the same degree of approximation, in the neighborhood of absolute zero. For that matter, the law of conservation of mass itself is inapplicable in the context of nuclear reactions, or if the velocity of expansion of the gas approaches that of light. (p. 96)

Kaplan concludes by asserting that all laws are bounded.

The notion of generalization is obviously relative. Newton's theory was more general than Galileo's. Einstein's theory is more abstract than Newton's. At what point are we willing to assert that we have indeed arrived at a "general law?" Physics has progressed at a rapid rate even though its covering law theories contain laws which are not universal. In short, it seems that if one is willing to admit that theory is culturally bound, then the problem raised by Monge is obviated. However, if one wishes to generalize across cultures and does find cultural variations, it is eminently possible to find laws which will explain cultural variations. Of course, one problem with cross cultural research is simply that what may appear to be "similar situations" between two or more cultures may not be "similar situations" at all. It seems reasonable to conclude that even if symbolic behavior is culturally bound, it is still possible to explain such cultural variations with general covering laws. Moreover, it is possible for theoretical propositions to be invariant from culture to culturebut the character of constructs employed in the theory to change from culture to culture. For

example, the lawlike generalization that increases in source credibility produce increases in persuasion may be invariant across cultures; however, the attributes that make a source highly credible in Chad may not be the same attributes that make for high source credibility in Costa Rica. The law holds in both cultures, but the relevant dimensions of the constructs load differently between cultures.

Temporal Variation

In addition to cultural variation, temporal changes in the behavior of a phenomenon might be thought of as presenting a problem to the covering law model. After all, we found that the covering law model assumes that its laws hold through time and space. If a phenomenon shows change through time, for example a "developmental process" of some kind, it would seem that the covering law model would not be able to adequately deal with the phenomenon. Such genetic explanations (Hempel, 1965) are quite common in both history and the behavioral sciences.

Here again we find that if one is to adequately explain why a developmental process occurs, sets of lawlike statements will have to be developed which explain why the phenomenon under study moves from one "system state" to another. For example, let us assume that in both cross sectional and longitudinal studies, in which we adequately control for cohort differences, we find that increasing age is strongly related to the ability to perform on concept attainment tasks. The older the child is, the better he or she is able to perform on such tasks. In view of our earlier discussion, this finding is not, by itself, very interesting because it has little, if any, explanatory power. As Brodbeck (1968) has cogently pointed out, age is a "non-variable" because it doesn't explain. It is not until we begin to try to explain why a child of five years of age can perform, say, twice as well

as a three year old on the average that the finding becomes scientifically interesting. Of crucial importance to the present discussion is the fact that in developing an explanation for the observed age differences, we will probably have to generate a set of lawlike generalizations or "process laws" (Bergmann, 1957) which explains the temporal change. These generalizations might involve the development of the nervous system. In any case, until such explanations are developed, the data regarding age differences are merely descriptive. Again, it seems that the covering law model is necessary for explaining change through time.

POTENTIAL BARRIERS TO THEORY CONSTRUCTION

Theory construction is essentially a creative process. Brodbeck (1968) points out that it is up to the theory builder to determine which theoretical statements will be the higher order axioms and which statements will be deduced from them as theorems. Of course, the theory builder must decide on the basis of previous research, hunch or dream exactly what constructs are likely to be the most important ones in explaining a given phenomenon. By its very nature, theory construction is a high risk venture. Since theoreticians are advancing conceptions of how a slice of physical or social reality works, they run the risk of being wrong. It is understandable, then, why some theorists become ego-defensive when faced with findings which contradict their theories. Their beliefs about the nature of the world have been questioned.

By contrast to theory building, testing a hypothesis derived from someone else's theory is a relatively low risk activity. Moreover, it seems reasonable to assert that hypothesis testing is generally a less creative activity than theory construction; although at times research design can demand considerable creativity. Perhaps the reason our discipline is heavy with hypothesis testers is that theory construction has been perceived as too risky to undertake. Hopefully, our discipline is arriving at the point where at least some persons will be willing to risk being wrong!

In addition to the perceived risky nature of theory construction, there are a number of other "mind sets" which act to discourage the development of covering law-type theories. I have been able to identify five such impediments to the development of covering law theories, but I am certain that there are more. In order to construct a covering law theory, the theory builder must overcome the following five obstacles.

Irrelevant Variety

It is a tradition that in their first chapters, authors of introductory psychology textbooks emphasize the complex nature of human behavior. As we look around us, we are prone to agree with the assertions of complexity these authors make. We have already considered the problem of cross cultural differences and their possible impacts on our ability to formulate general laws. However, it just may be that one reason we perceive human behavior to be very complex is that we do not understand it very well. I can remember clearly during the late 1940's what a mystery the disease polio was. Almost every summer we were prevented from swimming in public pools because it was felt that polio was spread in such environments. Cancer is today's medical mystery. It seems, however, that once the disease processes are understood, e.g. polio is caused by a virus from which persons can be immunized, the perceived level of complexity of the phenomenon decreases drastically.

In the behavioral sciences, which are still very young, we may be the victims of what I call irrelevant variety. Irrelevant variety is generated by the presence of attributes in a situation which have little to do with the phenomenon we are studying but which give the impression that what we are studying is "very complex." To illustrate, let us suppose that we are interested in studying digestion in humans and we observe that persons who live in North America sit on chairs when they eat, while persons who live in

Japan and Korea sit on the floor when they eat. We have observed a reliable cultural difference in eating habits. However, this kind of cross cultural difference may be totally irrelevant to the role played by bile in digestion. In all likelihood, bile behaves much the same in both cultures' members. Thus, the observed cross cultural difference introduces variety which is irrelevant to the phenomenon we are attempting to explain.

Although persons differ along an incredible number of physical, psychological and social dimensions, merely because this is so does not mean that all of these differences will make a difference in terms of the phenomenon we are studying. It is doubtful that the number of hairs on one's right arm, eye color, hair color, or cephalic index have much to do with one's susceptibility to persuasion. For many of the phenomena we study, it is probably the case that relatively few variables ultimately can account for most of the action. We just do not know what those powerful variables are yet. One reason for this state of affairs is that we are fooled, more often than not, by irrelevant variety.

Irrelevant Change

Since publication of Berlo's book The Process of Communication (1960), we have been persuaded that communication is best viewed as a process. Smith (1972) has pointed out that in a majority of cases, persons who do communication research do not employ research designs which capture the process nature of communication phenomena. Since the process notion implies that both physical and social reality are in a constant state of change, here again it would seem to be impossible to construct laws which are invariant through time. This particular problem was discussed earlier in the present paper.

However, as is the case with irrelevant variety, it is probable that not all change has an impact upon the phenomenon we are studying. That is, in

spite of the fact that the physical universe is in a state of change, certain physical laws still hold. Furthermore, it appears that these laws will continue to hold for some time to come. The same can be said for lawlike generalizations in the behavioral sciences. For example, in all probability perceived attitude similarity will continue to lead to high levels of interpersonal attraction; even though our society is in a constant state of change. Not all changes through time make a difference in what we study. Recognizing this possibility increases the likelihood that one will successfully build a lawlike generalization.

Realism versus Nominalism

When theorists go about the job of constructing a theory, they can choose to include in their theories only constructs for which there are empirical indicators (real constructs) or they can include some constructs for which there are not yet any operational definitions (nominal constructs). This is not only the case in the social sciences. The construct of mass in physics is a nominal one. The problem with including nominal constructs in a theory is simply that the theory becomes more difficult to test as the number of such constructs increases. Theoretical statements involving nominal constructs cannot be directly tested. Of course, there is always the possibility that what is a nominal construct at the present time will become measureable at some later time.

There are some good reasons for including nominal constructs in a theoretical system. Dubin (1969) has argued that their inclusion will encourage the development of new empirical indicators; that is, if theorists insist on including only real constructs in their theories they are not likely to search for new empirical indicators. A second argument advanced by Dubin concerns the ability of a theory to go beyond the "obvious." Here he argues that sole reliance on real constructs will ultimately lead to the study of relatively trivial problems. In general, then, the inclusion of some nominal constructs in a theory may have the effect of

making the theory more heuristically provocative. If the nominal constructions do not lead inquiry in new directions, however, then their inclusion in the theory must be questionned. Moreover, if the statements involving the nominal constructs appear to be very unlikely, then the constructs must be revised or excluded.

The crucial point to keep in mind is that useful theories can be constructed which contain nominal constructs. Nominal constructs can aid in the process of developing covering law explanations. Theorists should not hesitate to use nominal units in their theories if they can provide a plausible justification for so doing. In a real sense, one of the most imaginative and creative processes involved in theory construction is the development of a nominal construct.

Insensitivity to Embeddedness

The problem of irrelevant variety discussed above centered around the idea that when we are attempting to ascertain the relationships between two or more variables, we may be distracted from this effort by a host of situational attributes which are totally irrelevant to the relationships we are investigating. The presence of many such attributes leads us to believe that we are dealing with a "very complex phenomenon." There is a closely related problem concerning our sensitivity to the presence of a relationship across a number of different situations. For example, let us assume that we are interested in studying the relationship between leader dominance and the amount of conflict manifested in groups. Suppose that we do a series of studies in school classrooms in which we rate teachers on their levels of dominance and also rate their classrooms in terms of the amount of conflict manifested. Say we find that teachers who are highly dominant have less overt conflict in their classes than teachers who are less dominant.

Now let us assume that we try to study the relationship between the same two variables in a family situation. Obviously, when we move from the classroom

into the home situation, we are faced with a different kind of social context. For example, the history of relationships in the family situation is generally much longer than the histories of relationships in the school situation. Furthermore, and this is crucial, the ways in which both dominance and conflict are expressed in the two situations may be extremely different. Conflict may be more overt in the classroom than in the family. Or, dominance may be more subtle in the family than in the classroom.

Suppose our studies in the family situation fail to find any relationship between dominance levels and conflict. Can we confidently say that the dominance-conflict relationship does not hold in the family situation? Perhaps not. It may be the case that given other measures of dominance and conflict in the family studies, we might have found the same relationship between the two variables as we found in the classroom situation. What is being argued here is that general laws may be manifested differently in different situations. The law is generally true but the way in which it is embedded across situations is such that it looks "different" to the unknowing observer. For example, in the classroom the teacher might always primarily use verbal means to express dominance; while in the family situation, dominance might be expressed mostly through nonverbal means. The important point is that the lawlike generalization concerning the relationship between dominance and conflict might be generally true, but because the behavioral manifestations of it show variations across situations, we might be fooledinto believing that there is no generalization here.

The above discussion supports the necessity of "triangulation of measurement processes" discussed by Webb, Campbell, Schwartz and Sechrest (1966). Since most of the research done in our discipline, as well as other behavioral sciences, does not employ multiple empirical indicators for the same construct, it could be that we are not finding lawlike generalizations because we are not adequately measuring the variables we hope to put into a lawlike relationship with each other. It is imperative that we begin to require that multiple operations,

preferably nonreactive ones, be an integral part of our research.

Overemphasis on Statistical Techniques

There are at least two ways in which blind faith in statistical techniques can prevent us from constructing useful theories. The first problem concerns the use of such techniques as factor analysis as substitutes for thinking. The second problem is related to the notion that something has to vary before it is of interest to us. While I would be the first to encourage everyone who does communication research to become as sophisticated as possible in as many different statistical techniques as he can, it is also important to consider the connection between all of these techniques and the theory construction process.

As an undergraduate student I had a professor who once explained while describing factor analysis that you only get out of factor analysis what you put into it, garbage in-garbage out. Moreover, he made it clear that since this is the case, one cannot claim that the factors resulting from a factor analysis constitute some kind of "discovery." When one looks at the history of factor analysis, he sees that when investigators have attempted to employ the technique as a theory testing tool, the results have been somewhat disappointing. Spearman's two factor theory led to the development of factoring methods which maximized the chances of finding the g-factor; while Thurstone developed rotational schemes which increased the probabilities of finding several primary mental abilities. Given this kind of situation, it is obvious that what people "discover" through factor analysis depends heavily upon what brand of factor analysis they employ and how they label the factors they find! The same may be said for the cousins of factor analysis.

There are those who insist on relying on factor analysis and related techniques to "discover" constructs for communication inquiry. The general rationale for these investigators seems to be that if one makes up a large set of items, administers them to a large group of persons, and then factors the items, a number of useful theoretical constructs will be derived. It is of further interest to note that very few investigators who find these new factors then go on to employ these new variables in further research. They merely report the factor analysis and leave it at that. More important, however, is the fact that construct formation should precede the use of factor analysis and related techniques. Such techniques should not be employed as substitutes for conceptual definition and the explication of relationships among theoretical constructs. Once theoretical constructs have been formed and their linkages specified in a theoretical system, one should then become concerned with measurement development and employ factor analysis as a means for assessing the internal consistency of measures and the possible relationships between and among different empirical indicators of the construct.

A second problem with an overemphasis on statistical methodology concerns the fact that when we observe <u>patterns</u> of communication behavior which are invariant, or relatively so, we tend to discount their importance because no variance can be accounted for in such a phenomenon! In this case, the statistical model forces us to search for a variable so that these variables can be accounted for by other variables. However, it seems just as important, if not more important, to understand <u>why</u> persons manifest certain relatively invariant patterns of communication through time and space. It is paradoxical that on the one hand we say that human communication is a complex, interactive, etc. process, but when we see recurring patterns of communication we label them trivial and not worthy of study because the patterns do not vary much.

For example, we know that when persons meet for the first time, their conversations are dominated by exchanges of biographic and demographic information. Some observers might find this phenomenon relatively uninteresting. Although it is easy to understand why most naive observers might judge a conversation dominated by biographic and demographic information to be "superficial," this is the very recurrent kind of pattern which should pique the curiosity of behavioral scientists. They should ask, "Why do persons exchange that kind of information when they meet?" Instead, because the phenomenon is relatively invariant, behavioral scientists dismiss it because they cannot "correlate" it with anything else. This is equivalent to dismissing the law of falling bodies because it is invariant!

CONCLUSION

In this paper we have examined the structure of the covering law model, some criticisms of it, and some ways of thinking which impede the development of covering law theories. It should be emphasized that a defense of the covering law approach does not deny the usefulness of the rule governed approach or the systems approach. It is probably safe to say, however, that the covering law model does provide the most complete explanation of a phenomenon when the explanans are true. Moreover, the covering law approach encourages the systematic development of progressively more abstract theories through the subsumptive process. This process ceases when there is nothing else to explain (Brodbeck, 1968).

The reader should also keep in mind that the covering law model as described by philosophers of science does not describe how scientists actually behave (Hempel, 1965; Kaplan, 1964). The explanatory models described in the present paper are ideals. Anyone who does science, as opposed to merely talkingabout doing it, knows that liberal portions of chewing gum and bailing wire are frequently necessary to hold both theories and research together. This is not

only the case in the behavioral sciences; it is also true in the physical sciences. Braithwaite (1953) has asserted that persons will continue to use theories even when they know that the theories are only partially correct. He points out that long before Einstein advanced his theory of gravitation, it was known that Newton's theory could not account for the observed motion of Mercury's perihelion. It was not until Einstein's theory was available that Newton's theory was dethroned. Thus, the reader should not leave this paper with the idea that in order to be successful communication theorists all that we need do is "meet the assumptions" of the covering law model or any other mode of inquiry. There is considerable room for hunch, intuition, creativity and a host of other non-scientific sounding activities within the domain of scientific inquiry. Overly rigid adherence to any mode of inquiry is not only intellectually dangerous, but it also takes the fun out of scientific life.

References

Bandura, A. Principles of behavior modification. New York: Holt, Rinehart, and Winston, 1969.

Berger, C. R. and Calabrese, R.J. Some explorations in initial interaction and beyond: Toward a developmental theory of interpersonal communication. Human Communication Research, 1975, 1, 99-112.

Bergmann, G. Philosophy of science. Madison: University of Wisconsin Press, 1957.

Berlo, D. K. The process of communication. New York: Holt, Rinehart, and Winston, 1960.

Blalock, H. Theory construction: From verbal to mathematical formulations. Englewood Cliffs: Prentice-Hall, 1969.

Braithwaite, R. B. Scientific explanation: A study of the function of theory, probability and law in science. London: Cambridge University Press, 1953.

Brodbeck, M. Readings in the philosophy of the social sciences. New York: Macmillan, 1968.

Campbell, D. T. Personal communication. August, 1975.

Cappella, J. N. A derivation and test of a cybernetic, coorientational model of interpersonal communication. Paper presented at the annual convention of the International Communication Association, April, 1974, New Orleans.

Cushman, D. and Whiting, G. C. An approach to communication theory; Toward consensus on rules. Journal of Communication, 1972, 22, 217-238.

Dray, W. Laws and explanation in history. London: Oxford University Press, 1957.

Dubin, R. Theory building: A practical guide to the construction and testing of theoretical models. New York: Free Press, 1969.

Duncan, O. D. Path analysis: Sociological examples. American Journal of Sociology, 1966, 72, 1-16.

Gardiner, P. L. The nature of historical explanation. London: Oxford University Press, 1952.

Garfinkel, H. Studies in ethnomethodology. Englewood-Cliffs: Prentice-Hall, 1967.

Goffman, E. The presentation of self in everyday life. New York: Anchor, 1959.

Harre, R. and Secord, P. F. The explanation of social behaviour. London: Blackwell, 1972.

Hawes, L. C. Pragmatics of analoguing: Theory and model construction in communication. Reading, Mass.: Addison-Wesley, 1975.

Hempel, C. G. Aspects of scientific explanation: And other essays in the philosophy of science. New York: Free Press, 1965.

Kaplan, A. The conduct of inquiry: Methodology for behavioral science. San Francisco: Chandler, 1964.

Monge, P. R. Theory construction in the study of communication: The system paradigm. Journal of Communication, 1973, 23, 5-16.

Nagel, E. Assumptions in economic theory. American Economic Review Supplementary Volume, 1963, 211-219.

Parks, M. R. Toward an axiomatic theory of relational communication. Paper presented at the annual meeting of the International Communication Association, April, 1975, Chicago.

Pearce, W. B. Consensual rules in interpersonal communication: A reply to Cushman and Whiting. Journal of Communication, 1973, 23, 160-168.

Smith, D. H. Communication research and the idea of process. Speech Monographs, 1972, 39, 174-182.

Toulmin, S. Concepts and the explanation of human behavior. In T. Mischel (ed.) Human Action. New York: Academic Press, 1969.

Webb, E. J., Campbell, D. T., Schwartz, R. D., and Sechrest, L. Unobtrusive measures: Nonreactive research in the social sciences. Chicago: Rand Mcnally, 1966.

Wright, S. The method of path coefficients. Annals of Mathematical Statistics, 1934, 5, 161-215.

Wright, S. The interpretation of multivariate systems. In O. Kempthorne, et al. (eds.) Statistics and mathematics in biology, 1954, 11-33.